Solve problems and answer questions instead of following trends!

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Abstract: With this comment, I comment on the key ideas of the opinion paper by Ocklenburg al. The authors describe trends in lateralization research for the next decade. With my commentary, I take the liberty of pointing out that it is first more important to focus on the relevant questions to be answered in the context of lateralization research before calling out research trends. Furthermore, the focus of lateralization research in humans should be more on the human brain and human behaviour because the human brain is highly specialized despite many similarities with other species' brains.

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COMMENT

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Solve problems and answer questions instead of following trends!

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ABSTRACT

With this comment, I comment on the key ideas of the opinion paper by Ocklenburg al. The authors describe trends in lateralization research for the next decade. With my commentary, I take the liberty of pointing out that it is first more important to focus on the relevant questions to be answered in the context of lateralization research before calling out research trends. Furthermore, the focus of lateralization research in humans should be more on the human brain and human behaviour because the human brain is highly specialized despite many similarities with other species' brains.

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In their opinion article entitled "Laterality 2020: entering the next decade", Ocklenburg et al. describe and conclude "which trends are likely to shape the next decade of laterality research by opening the way for novel questions, enhancing collaborations and boosting the reliability and validity of research findings in our field". Since the authors have chosen a very promising title and have optimistically highlighted the proposed influence of these trends, I take the liberty of discussing this opinion paper somewhat more critically.

Before I start my comment, I would like to emphasize that this opinion paper's authors have provided a good overview of current lateralization research. Nevertheless, some, in my opinion, essential points are unmentioned or have not received the necessary attention.

I admit that I was excited when I first read the title and abstract and was expecting new insights and perspectives into the future of laterality research.

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Over the last two decades, research on laterality has become less prominent in both cognitive neuroscience and neuropsychology research. Until the early 1990s, laterality research was considered a central discipline of cognitive neuroscience. The advent of modern imaging techniques and the currently available methods for analyzing neuroscientific data has shifted this focus, and new questions (that are considered more important than laterality problems) have attracted the attention of researchers worldwide.

Although numerous published papers have covered a wide range of topics, in the fields of cognitive neuroscience, general neuroscience, neurology, and psychiatry, these reports generally do not investigate asymmetries specifically but, instead, examine a variety of different issues (e.g., the neural underpinnings of several psychological domains, including auditory, visual, speech, and emotional processing domains, and the neural foundations of exceptional abilities and aging). I estimate that roughly 50% of these papers have reported some type of anatomical and/or functional asymmetry. However, these findings have not been considered (obviously) relevant to classical asymmetry researchers, whose main interests are focused on research that explicitly deals with asymmetric findings. Consequently, this abundance of findings has been, unfortunately, generally neglected by classical laterality researchers. Interestingly, this branch of research has already applied several of the future trends that were mentioned for laterality research in the Ocklenburg et al. opinion paper (e.g., applying big data, meta-analyses, machine learning, and graph-theoretical approaches).

I was, therefore, very interested in what Ocklenburg et al. were presenting and whether they would pave the way for new and interesting ideas to approach the unsolved topics of the (human) brain and behavioural asymmetries. I must admit that I was somewhat disappointed by what the authors described as trends in modern research on laterality research. Their article primarily presented a plethora of superficial and general statements about possible future efforts in laterality research. Most of the 10 points they raised can easily be adapted (using only minor word changes) to almost any psychological or cognitive neuroscientific discipline. One could easily extend this trend list by adding other "trends" (e.g., computational neuroscience, modelling, and simulation), followed by "for laterality research". Does the addition of the phrase "for laterality research" actually increase the importance and productivity of laterality research?

At the climax of neuroimaging research development, in the 1990s, the famous cognitive psychologist Steven Kosslyn summarized an emerging problem with the elegant phrase, "If brain imaging is the answer, what is the question?" (Kosslyn, 1999). After reading the article by Ocklenburg et al., I rephrased Kosslyn's statement by asking, "If all these trends are realized, what will be the answer for laterality research?"

To reinstate the importance and impact of laterality research, we should first formulate appropriate study questions and then identify appropriate methods and approaches to answer them. In Ocklenburg et al., only a few explicitly formulated questions for current and future laterality research were presented: What are the genetic and non-genetic factors for asymmetries? How do molecular processes mediate between environmental factors and functional and structural hemispheric asymmetries? How are lateralization patterns identified in different species or classes of animals, and how are they associated with each other?

In addition to these truly important questions, I only found vaguely formulated questions, hidden in descriptions of recent studies and suggestions for new experimental and observational approaches. We are currently learning that laterality research requires large data sets (which is true, but what about those rare, exceptional subjects who present with exceptional anatomical and functional asymmetries?), more meta-analyses (but which studies require this?), more ecologically valid behavioural studies (for what purpose or focus?), we have to use machine learning (for what purpose or focus?), graph-theoretical approaches (to explore which questions?), neurofeedback (for what purpose?) and mobile EEG recordings (for what purpose?). Although these approaches and techniques are interesting, which questions should we apply them to? In other words: What problem in laterality research will we solve using these nice and cool ideas?

In my view, the first step should be the formulation of appropriate questions to resolve the unanswered and poorly understood laterality issues. As an established researcher who began his academic career 30 years ago, performing studies on anatomical and functional asymmetries, I recognize that many unanswered (but still fundamental) questions remain that should be addressed in the future. For example, we still do not know why the human brain is asymmetrical, in terms of volume, shape, cortical surface, cortical thickness, and the architecture of the cortical and subcortical fibre connectivity system. We also do not know why the human brain is asymmetric at all and whether asymmetry is a mere consequence of genetic influences or whether other biological or environmental influences are involved. Is the exceptional anatomical and functional asymmetry of the human brain a consequence of brain size? What are the relationships between structural and functional asymmetries? Which of the so-called functional asymmetries are important, and which asymmetries are a consequence of environmental influences, learning, or random chance?

I am not going to write my own opinion paper, nor do I have any suggestions for further research "trends"; however, I do suggest that future laterality research borrow more ideas from current cognitive neuroscientific research and determine how to reconcile these ideas with modern cognitive neuroscientific facts. In addition to the important study questions I

mentioned above, more focus should be placed on the dynamics of anatomical, neurophysiological, and behavioural asymmetries and how these asymmetries are shaped by environmental and biological factors. Finally, the ability to associate the findings from laterality research with both clinical and non-clinical applications is of utmost importance, and this effort should not be limited to psychiatric and neurological developmental disorders but should be extended to all brain-based and brain-related disorders.

Ocklenburg et al. also report on what they consider to be "tremendous" insights and progress in understanding laterality issues, which has been uncovered in the last decade. In this context they almost exclusively summarize genetic and anatomical studies from the animal kingdom, mostly of birds, rodents and fish. Although these findings are interesting and could be used as models or hypotheses for the study of laterality in humans, one should bear in mind that the human brain is very specific in terms of neuroanatomy, and neurophysiology (number of neurons, interconnectivity, neuronal packing density, etc.) (Roth & Dicke, 2005). In addition, the human brain generates and controls highly lateralized psychological functions that are not found in the animal kingdom (e.g., speech, complex motor functions). Therefore, I am sceptical whether the "tremendous" findings from animal research will really help to understand the extraordinary lateralization in humans.

Laterality research is also not an isolated island but an integral part of the entire cognitive neuroscience and neuroscientific discipline. Laterality research does not require "trends". Instead, this field requires clearly formulated questions, well-designed experiments, and studies that apply the best methods available to answer these questions. Only in this way will we be able to solve the unsolved questions in this interesting research area.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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